

says:—"Then, I imagine, the flow of heat through the gas will take place *as though* there were, in contact with each solid surface, a layer of gas whose temperature is throughout the same as that of the contiguous solid, and whose thickness is equal (or at least proportional) to the mean length of path of the molecules." Without these layers of uniform temperature or whatever may produce an equivalent effect it follows directly from Prof. Foster's reasoning that the rate at which heat is communicated is, as I maintain it is, independent of the density, whereas *if there were* any such layers I should at once admit the force of Prof. Foster's reasoning. The whole question turns therefore on the existence of these layers of uniform temperature.

Now what evidence of such layers have we? No experimental evidence certainly; and not only has the kinetic theory not as yet been applied to explain their existence but it is easy to demonstrate that according to this theory no such layers or any equivalent can exist. For in order that the condition of heat may remain unaltered it is necessary that the rate at which heat is transmitted across all surfaces parallel to the solid surfaces which can be drawn through the gas should be the same. And the rate at which heat is transmitted is for small variations of temperature proportional to the degradation of temperature, hence if there were a layer of uniform temperature no heat could be transmitted.

It is surely incumbent on Prof. Foster in assuming the existence of these layers to give some sort of proof in support of his assumption, but not one word does he say!

I cannot allow this to pass without pointing out that the description which Mr. Stoney has given of my view is grossly wrong and is certainly not to be gathered from anything I have written. Mr. Stoney carefully turns my position. He makes out that I have explained the action in question as *arising from convection currents*, whereas I have from first to last maintained that it is these currents which oppose and eventually overcome the action. He makes out that my theory takes no account of molecular motion, whereas, in truth, it takes no account of anything but molecular motion, the effect of the expansion of the gas being so obviously trivial that I have never even mentioned it.

Your readers may judge of this by comparing the first of the following quotations, which is from Mr. Stoney's letter, with the others which are from my own papers, and are the only expressions, not mathematical, which I have given of my views as to action in the question:—

Mr. Stoney's.

"Prof. Osborne Reynolds's explanation is based on the fact that when a disc with vertical sides is heated on one side and exposed to a gas, a convection current sets in, which draws a continuous supply of cold gas into contact with the hot surface of the disc. As this cold gas reaches the disc it is expanded, and thus its centre of gravity is thrown further from the disc. Accordingly, the disc, if freely suspended, will move in the opposite direction so as to keep the centre of gravity of the gas and disc in the same vertical line as before, and, if not freely suspended, will suffer a pressure tending to make it move in that direction. If I have understood Prof. Reynolds aright, this is both a correct and full description of his explanation as last presented."

My Own.

"Whenever heat is communicated from a hot surface to gas, the particles which impinge on the surface will rebound with a greater velocity than that with which they appropriate; and consequently the effect of the blow must be greater than it would have been had the surface been of the same temperature as the gas."

"And, in the same way, whenever heat is communicated from a gas to a surface, the force on the surface will be less than it otherwise would be, for the particles will rebound with a less velocity than that at which they approach."

"These forces arise from the communication of heat to or from the surface from or to the gas. These forces will be directly proportional to the rate at which the heat is communicated; and since this rate has been shown by Prof. Maxwell to be independent of the density of the gas, these forces will be independent of the density of the surrounding medium, and their effect will increase as the density and convection-currents diminish."²

The first of the quotations from my papers is followed by a mathematical expression on which I have depended for completeness, and from this expression, in which neither convection currents nor the expansion of the gas have any place whatsoever, it follows that whenever heat is steadily diffusing *into* or *through* a gas, the momentum transmitted across any surface in the direction in which the heat is diffusing will be greater than that transmitted in the opposite direction by a quantity proportional to the rate at which the heat diffuses, divided by the square root of the absolute temperature of the gas.

As to the value of what follows in Mr. Stoney's letter, I must leave it to your readers to decide. He proceeds to claim that his own theory has the advantage of being based on molecular motions, he says:—

"My explanation, on the other hand, is based on *molecular* motions which go on in the gas without causing any molar motion, and is independent of convection currents."

Then having thus attributed to me an explanation, I never even thought of offering, and having assumed the true base of my theory as alone belonging to him, he proceeds to show wherein I am wrong. And in every subsequent position which he attributes to me, he is as wrong as he is in his first statement.

Under these circumstances it would be useless for me to enter upon questions as to how far "diffusion," according to the kinetic theory may be more "sluggish" than Mr. Stoney's "penetration," or to discuss further the possibility of his "Crookes's layers."

In my last letter I showed that the condition of a gas which Mr. Stoney called a "Crookes's layer" was impossible, and I do not see that Mr. Stoney has improved his position by showing that he had arrived at the possibility of the condition by making the false assumption "*that gas is a perfect non-conductor of heat.*"

Wherein Mr. Stoney's views are at variance with the results of the laborious investigations of Maxwell, Clausius, Thomson, and others, he may best convince himself by referring to the works of these authors. Until he has read my papers and explained the discrepancies between his views and the generally-accepted laws of gases, I do not see that we have any common ground for discussion.

OSBORNE REYNOLDS

November 30

Mr. Crookes and Eva Fay

If Mr. Wallace had read my letter in NATURE of November 29 with a little more attention, he would have seen that I did not refer to the *Daily Telegraph* "as an authority in a matter of scientific inquiry," but that the account I gave of Mr. Crookes's "scientific tests" was given in *Mr. C.'s own communication to the 'Spiritualist'*, which would have been reproduced without abridgment if the columns of NATURE could have admitted it.

What I hold myself pledged to show (in NATURE, if it pleases, as well as in the new edition of my Lectures) is that the "tying-down by electricity" described by Mr. Crookes in the *Spiritualist*, is no more effective in preventing the performance of juggling tricks than Eva Fay's ordinary tying-down under which her tricks were publicly reproduced two years ago by Messrs. Maskelyne and Cooke. And since Mr. Crookes made no mention of the *extraordinarily* sensitive galvanometer he used, which is described for the first time by Mr. Wallace in the last number of *Fraser*, I only consider myself bound to show the *method* by which, with *ordinary* apparatus, the electric test may be evaded—the trained skill of the acute *trompeuse* being very probably required to meet the more severe test now first specified.

Mr. Wallace seems to me to have been a little hasty on another point. "The supposed *exposure* of Eva Fay in America," he says, "was no exposure at all, but a clumsy imitation." As this is merely Mr. W.'s *dictum* founded upon an imperfect newspaper report, I prefer to trust the judgment of the eye-witnesses who have publicly testified to the *completeness of the exposure*. Among these are not only three of the ablest men in New York (the Rev. Dr. Bellows, Ex-Surgeon-General Mott, and Dr. Hammond), but the reporters of the very newspaper referred to which had previously shown a decided leaning to the claims of spiritualism. And their judgment is confirmed by the fact (which Mr. Wallace probably considers as a newspaper fiction, but of which I have independent testimony) that *Eva Fay was forced by the local authorities to take out a licence as a juggler as a condition of the continuance of her public performances*.

The fundamental difference between Mr. Wallace and myself as to the validity of testimony in regard to the "occult" comes out so strongly in this case that we have really no common

¹ *Proceedings*, Royal Society, 1874, p. 407.

² *Phil. Mag.*, November, 1874, p. 3.

ground for a discussion which I cannot consider it profitable to continue.

WILLIAM B. CARPENTER

The Glacial Geology of Orkney and Shetland

OWING to an accident I did not see your number of September 13 containing my letter on the glacial geology of Orkney and Shetland and Prof. Geikie's article (vol. xvi. p. 414), until my return from Scotland a few days ago. Otherwise I should have troubled you sooner with a few observations thereon.

In the first place I wish to thank Prof. Geikie for the very courteous manner in which he has referred to the remarks of an outsider who has ventured to intrude on what the Professor has made, to such an extent, his own peculiar province.

In the next place I am glad to find that upon what was the most important fact in my statement, viz., the absence of raised beaches or other signs of recent elevation of the land in Orkney, Prof. Geikie agrees with me.

I call this the most important because it bears directly on the theory of wide-spread changes in the relative level of sea and land owing to secular causes, such as a change in the axis of the earth's rotation, or in the position of its centre of gravity. If it can be proved that the difference of level, which caused the raised beaches of the south of Scotland, and extended north along the coast of Ross and Sutherland, dies out as we proceed further north, and disappears altogether in Orkney and Shetland, it is truly a crucial experiment which shows that these raised beaches are due to local elevations of the land, and not to a general sinking of the sea.

This is the conclusion to which Prof. Geikie points, though he naturally finds it difficult to understand why the upheaval, so marked in Sutherland, did not affect Caithness and Orkney.

I believe I can add a few facts which may assist in removing these doubts.

At one of the places in Caithness mentioned by Prof. Geikie, where the existence of a raised beach might be possible, viz., in the sheltered Bay, between Freswick and Wick, I believe there is one, though less strongly marked and at a lower elevation than those in similar situations in Sutherland. I allude to a terrace which bounds the links of Keiss Bay, about half a mile inland from the present coast-line. I cannot speak positively, not having seen it for some years; but my recollection is that it is a perfect miniature reproduction of the terraces round Brora and other bays in Sutherland. If so, it is a positive proof that the elevation of the land died out towards the north, and we might reasonably suppose that somewhere about the line of the Pentland Firth was the neutral axis, on one side of which the land rose, while on the other it fell.

Be this as it may, the fact is, I think, incontrovertible that Orkney did not share in the southern movement of elevation. This rests not only on the absence of raised beaches, forming terraces, which might possibly have disappeared, but still more on the absence of all traces of marine action, such as pebbles, sand, or shells, on the low plains which must have been submerged.

I would ask Prof. Geikie to consider whether the single instance of the Loch of Stennis is not conclusive. If the sea had ever stood twenty or thirty feet higher relatively to the land than it now does, the whole plain up to the hills must have been a sheltered, shallow, inland fiord.

As the land rose to its present level this must have left not only a terraced beach at the foot of the hills, which might possibly have disappeared (though it is hard to see why it should have done so in such a sheltered situation), but the whole plain must have been a raised sea-bottom, strewn over with pebbles, sand, and shells. These could not have disappeared, and as they are nowhere visible and the plain consists everywhere of the ordinary rock, with a thin mantle of soil resulting from its disintegration by ordinary atmospheric causes, I am, I think, justified in assuming it to be proved that Orkney did not share in the recent movement of elevation which affected the rest of Scotland.

Now one word as to glaciation. I can assure Prof. Geikie that I do not think for a moment of setting my authority against his, and that if he is right in the instances of glaciation he tells us he has observed in Orkney, so far from being disappointed, I shall be pleased, for it will clear up what has long seemed to me a perplexing anomaly.

Of course Orkney must have experienced the full rigour of the glacial period, and it is only natural to expect that it should show the same abundant signs of glaciation as the adjoining counties of Scotland. Prof. Geikie will therefore excuse me if

I still retain a little of that healthy scepticism which is so conducive to the establishment of truth, and venture to plead that judgment may be suspended until there is further evidence. I do so mainly because the Professor's own statement is that during his visits to Orkney his attention was devoted mainly to the old red sandstone, and his remarks on glaciation were only incidental. Now there are some proofs of glaciation which are so obvious that there can be no mistake about them, others which may easily be mistaken, and which require close examination by a practised eye directed specially to them, to arrive at a just conclusion.

Boulders of foreign rock, perched blocks, rocks unmistakably rounded and polished by the ice plane, are among the former. But striæ require great practice and careful examination to be sure of them in a district of finely laminated sandstones which weather constantly into parallel lines or grooves. Stony clay again, from disintegrated rock, is often so like boulder clay that it requires close observation to distinguish one from the other. And finally where steep hills have crumbled away and filled up many places in the narrow valleys between them with their debris, as at Hoy, the appearances are very like those of glacial moraines.

Now I observe that nearly all the conclusive proofs of glacial action are wanting in Prof. Geikie's enumeration. He has not seen, or heard of anyone who has seen, a single boulder or perched block, or even a single piece of foreign stone in Orkney.

As regards boulder-clay I would join issue on his instances, taking especially that of Kirkwall Bay, because it is typical of the other cases and so easily accessible that the facts can readily be verified.

I believe it to be disintegrated and not boulder clay, for the following reasons:—

1. The clay is not compact like that of genuine boulder-clay, but of looser structure, and often clearly made up of minute splinters of the disintegrated rock.

2. The stones in the clay are never foreign stones, and are not scattered irregularly, as if shot out into a huge rubbish heap, as in true boulder-clay, but arranged for the most part so that the original lines of stratification can be followed.

3. If the section which resembles boulder-clay be followed up, it will be found to merge insensibly in what is unmistakably the common disintegrated surface soil of the district.

There only remains the question of *roches moutonnées*, and here I speak with the greatest diffidence, for certainly Prof. Geikie ought to know a great deal better than I whether a hummock of rock is or is not "admirably ice-worn and striated" like those behind Stromness.

I can only say that I have looked at them often, and they appear to me to be very different from the *roches moutonnées* of which I have seen so many in Scotland, Wales, and Switzerland. They are not rounded, smooth, and polished, as if planed into shape by some gigantic tool, but simply irregular hummocks of rock, sometimes smooth and sometime rough, according to accidents in the bedding and weathering of the strata. So at least they seem to me, and even in the valleys of Hoy, where, if anywhere, there were local glaciers, the sections shown by the small streams and low coast-line, always, I believe, exhibit the same appearance of sandstone strata, coming at an angle to the surface, and with their edges not planed off, but passing gradually into surface soil by disintegration.

Of course I make these statements subject to correction. It may be that I have failed to see things because my eye is not sufficiently educated. But when we couple what is, I believe, absolutely certain, viz., the absence of the more prominent and obvious proofs of glaciation in the form of boulders and foreign rocks, with the equally certain fact that Orkney was an exception to the general rule of recent elevation, I think Prof. Geikie will admit that the interests of science will be promoted by any remarks which may lead to reasonable doubts, and therefore to conclusive investigation, as to the fact whether Orkney does or does not give proof of having been covered by a great polar ice-sheet during the glacial period.

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Explosions

I HAVE been waiting to see if Mr. Galloway's paper on "Explosions in Mines" published in NATURE, vol. xvii. p. 21, would lead to any correspondence. Your readers may be interested in an incident reported to me by the late Dr. Böttinger, of Messrs. Allsopp's brewery, Burton-on-Trent,